Had the breath been hotter than the mouth the instrument could not have failed to register a higher temperature than 98·4°, but being really cooler, the instrument, of course, recorded a lower temperature.

What is then the true explanation of the phenomenon observed by Dr. Dudgeon? I believe that it is simply an example of the conversion of latent into sensible heat by the rapid condensation of aqueous vapour. The organic fabrics which compose our clothing are all more or less hygroscopic—that is to say, they have the capacity of imbibing aqueous vapour and condensing it into the solid and liquid forms. The expired breath is heavily charged with aqueous vapour; and aqueous vapour, at the moment of condensation, liberates an enormous amount of latent heat, which thus becomes sensible to the thermometer. In this particular watery vapour exceeds far away all other gases.

The following experiments were made with a view of testing the correctness of this view. Two strips of flannel were prepared, each six inches long and an inch and a quarter wide. The first strip was rolled, without any preliminary preparation, round the bulb of a clinical thermometer. The bulb, thus enveloped, was inserted between the closed lips, and the expired air was forced through the porous material for a period of five minutes. The thermometer rose to 104°. The instrument was then allowed to cool, and, after having been re-set, was again inserted between the lips, and breathed through for a second period of five minutes. This time the temperature only rose to 101°. The experiment was repeated a third time for a similar period, but this time the thermometer did not rise above 98·6.

These results tallied exactly with the requirements of the condensation hypothesis. During the first period the fresh dry flannel absorbed and condensed the watery vapour passing through it with such rapidity that the liberated sensible heat was sufficient to raise the mercury several degrees above the temperature of the mouth. In the second period of five minutes the hygroscopic activity of the flannel had been greatly reduced by the previous absorption of aqueous vapour, and the thermometer only rose slightly. In the third period saturation had been approached, and the breath passed through the flannel almost without depositing any of its moisture, and accordingly the thermometer only indicated a temperature slightly higher than that of the mouth.

The second strip of flannel was subjected to a little preliminary preparation. In order to increase its hygroscopic activity it was thoroughly dried (superexsiccated) by holding it for a few minutes before the fire. When it had cooled down to the temperature of the room it was wrapped round the bulb of the thermometer, and the experiment was proceeded with as before. The result surprised me. *In one minute* the mercury had risen not only to the top of the scale (112° F.), but had filled the little bulb above it, that is to say, it had risen to at least 115° F. When the instrument had cooled it was reset, and inserted again between the lips and breathed through for three minutes. At the end of this time the scale marked 106° F. After the instrument had been cooled and reset the experiment was repeated a third time, and the temperature only reached 102° after breathing through the envelope for four minutes. A fourth trial of four minutes only produced a record of 98·4°. Here again the development of heat steadily declined as the flannel became less hygroscopic.

It is probable that, with the superexsiccated flannel the first portions of aqueous vapour condensed at the beginning of the experiment pass at once from the gaseous into the solid form, and constitute that portion of water which is incorporated in intimate union with all organic tissues. This accounts for the extreme rapidity of the development of heat at the commencement of the experiment. I found that even a single long expiration through the freshly-warmed flannel raised the mercury to 110° F.

Dr. Dudgeon's observation will not necessitate a revision of our conclusions respecting the temperature of the breath, but he has supplied us with an exceedingly elegant and easy way of demonstrating the liberation of sensible heat which takes place during the passage of water from the gaseous into the solid and liquid state. WM. ROBERTS

Manchester, November 10

Height of the Aurora

IN NATURE, vol. xxii. p. 291, is inserted a letter of Mr. T. Rand Capron, on the determination of the height of auroræ, wherein I read: "It is unfortunate that simultaneous observations of the auroral corona are almost entirely wanting. I . . . would be glad if any particulars could now be furnished me."

Having treated the subject of the auroræ and their properties in an ample manner in my "Théorie cosmique de l'Aurore polaire" (*Memorie della Società degli Spettroscopisti Italiani*, 1878, vol. vii.), wherein I have adduced proofs of the thesis that *The corona is an optical illusion, due to the laws of celestial perspective*, I was astonished to find the alleged words used by so great an authority. That "simultaneous observations" of the auroral corona will be ever without any result, as far as its height above the earth is concerned, follows already from the known property, that the corona always shows itself in the direction of the *local* magnetic total force (given by the inclination needle).

Regretting that such a well-established fact seems not generally known, I take the liberty to refer Mr. Rand Capron to the chapter of my treatise, "Dans quelle Région de l'Atmosphère terrestre se trouvent les Rayons de l'Aurore polaire, et est-ce que la Couronne est une Chose réelle?" and will repeat here that very beautiful determinations of the height of streamers and beams were obtained by Prof. Heis and Dr. Flögel, and by Prof. Galle in Germany, showing a height of the phenomenon from 20 to 100 miles (of 15 in 1 degree). These results are published in the *Zeitschrift der oesterr. Gesellsch. f. Meteor.* vii. p. 73.

I regret to have found no earlier opportunity of answering the request of Mr. Rand Capron, but think that this letter may still have some interest, notwithstanding the valuable article by Mr. Plummer in NATURE, vol. xxii. p. 362.

Groningen (Netherlands), H. T. H. GRONEMAN
November 10

Fascination

As a contribution to this subject, at least of new material if of no decisive evidence in support of any existing theory, I offer the conclusions which Malachi Foot, Member of the College of Physicians and Surgeons, N.Y., reached in 1807 relative to this matter. A short memoir of his which I recently met was published in the *Medical Repository* for that year, entitled "An Examination of Dr. Hugh Williamson's Memoir on Fascination, to which is subjoined a New Theory of that Phenomenon," and is striking both in matter and conception.

The author, after displaying some temper over Dr. Williamson's willingness to attribute the well-accredited effects of snake-charming to terror, producing in the victim a condition which he (Dr. Williamson) terms "dementation," and "wherein extreme fear stupefies the mind and deprives him of the understanding," produces his own explanation. Although he acknowledges the paralysing effects of fright, and instances quadrupeds falling lifeless from the effect of fear, deer stricken motionless by the light of a torch, &c., yet he inveighs against the false reasoning which discovers in these cases of arrested volition any analogy to the phenomena of so-called fascination. Our author, evidently of no superstitious habit, distinctly admits the fact that the snake repeatedly captures prey by a method seemingly so occult as to merit the characterisation of fascination, and develops his theory in the light of that very thought.

He ascribes to the primary sensations of animals, in them unmodified by reflection as to their source or character, complete efficacy to awaken emotions of pleasure of an intensity to us quite incommensurate with the apparent causes which evoked them—emotions so powerful as to absorb all other secondary feeling, enfolding the animal in a delightful but numbing trance, whose stages advance from attention through ecstasy to anæsthesia. And he finds in the eye a sensory centre which most expansively responds to all outward stimuli. This much premised, he applies it to the case in hand. The snake, fixing its glittering eyes with hungry expectancy upon its victim, at the same time throws its body into graceful curves and raises its tail, undulating with a soft and inviting motion. (Foot insists upon the almost invariable accompaniment of motion as auxiliary in attracting and pleasing the prey.) The bird's eye, once caught, becomes ensnared in the endless succession of contortions, and it draws near, dominated by simple delight.

As Foot expresses it, "the pleasurable movements of the organs of vision stimulate to approach and excite an eager desire to embrace." *Reverie oculorum* ensues, and the bird flutters helpless to the ground. Foot speaks of having seen a cat succeed in similarly charming birds by wreathing the body and waving the tail. He might have confounded this with the

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ordinary nervous concentration of attention common to the Felidæ before "pouncing." He speaks of larks being attracted in the South of France by means of an octagonal box holding a mirror mounted on a pivot which is turned by the wind. The reflected rays of sunlight dazzle and delight the birds, and they approach near enough to be caught by a spring net. The preliminary phase, that of attention, wherein curiosity perhaps predominates, is illustrated in the known trick of a fox amusing ducks by rolling itself down a bank, as also in the perilous interest excited in a loon by a handkerchief waved by an unseen hand. Many must have experienced, on looking over very high galleries upon floors beneath, or over sheer precipices, an almost uncontrollable impulse to throw themselves headlong down. Can this feeling be described as akin to "fascination"?

L. P. GRATACAP

Amer. Mus. Nat. Hist., N. Y., October 28

A. PERCY SMITH.—The little centipede is *Geophilus electricus*, well known to be occasionally luminous.

HOMAGE TO MR. DARWIN

ON Wednesday, November 3, a deputation from the Yorkshire Naturalists' Union waited upon Mr. Darwin at his residence, Down, Beckenham, Kent, for the purpose of presenting him with an address expressive of admiration for his long devotion to scientific research, and appreciation of the great and important results to which his investigations have led. Prof. Williamson, F.R.S., of the Owens College, Manchester, who is the president of the Union for the current year, was prevented from accompanying the deputation by the pressure of his professorial duties. The deputation arrived at Mr. Darwin's residence about 1 p.m., and was received in a most hearty manner by the great naturalist himself, Mrs. Darwin, and other members of the family. The members of the deputation were introduced individually to Mr. Darwin by Dr. Sorby, vice-president of the Union, and then the interesting ceremony of the presentation of the address was at once proceeded with. After a few words on the work of the Union by Dr. Sorby, the address was read by Mr. Thomas Hick, B.A., B.Sc., and formally presented to Mr. Darwin by Dr. Sorby. Replying to the address, Mr. Darwin assured the deputation of his deep sense of the honour the Yorkshire Naturalists' Union had conferred upon him on that occasion, and only regretted that he had not done something more deserving of such an honour. He had no idea previously that there was so strong a body of working naturalists in Yorkshire, but was pleased to learn that such was the fact, and to find from the *Transactions* that had been forwarded to him that they were doing useful work. Coming from such a body, the address was all the more gratifying to him, though he still feared he hardly merited the good things that had been said of him. The address which had been presented to him he and his family would for ever treasure and preserve, and he desired to express his warmest thanks, both to the deputation and those whom they represented, for it, and for the kind and considerate manner in which everything connected with it had been arranged. Subsequently the deputation were entertained at luncheon, and having spent a short time in familiar conversation with their hospitable host and his family, took their departure amid mutual expressions of kindness and regard.

The following is the text of the Address, which is dated August last:—

To Charles Darwin, LL.D., M.A., F.R.S. &c., &c.

SIR,—The Council and Members of the Yorkshire Naturalists' Union, all of whom, with scarcely an exception, are working students of one or more of the various branches of natural history, desire to express to you in a most respectful manner, and yet with the greatest cordiality, their admiration of your life-long devotion to original scientific research and their high appreciation of the almost unparalleled success of the investigations by which you have contributed so largely to the modern development and progress of biological science.

More especially do they desire to congratulate you on the fact that your great work on the Origin of Species will come of age at an early date, and that your life has been spared long enough to enable you to see the leading principles therein enunciated accepted by most of the eminent naturalists of the day. On the conspicuous merits of that and of your other published works they need not dwell, as those merits have been recognised and admitted even by those who have dissented most strongly from the conclusions at which you have arrived. They may nevertheless be permitted to remind you that your writings have been instrumental in giving an impetus to biological and palæontological inquiries which has no precedent in the history of science, except perhaps in that which followed the promulgation of the gravitation theory of Newton, and that which was due to the discovery of the circulation of the blood by Harvey.

One of the most important results of your long-continued labours, and one for which you will be remembered with honour and reverence as long as the human intellect exerts itself in the pursuit of natural knowledge, is the scientific basis you have given to the grand Doctrine of Evolution. Other naturalists, as you yourself have shown, had endeavoured to unravel the questions that had arisen respecting the origin, classification, and distribution of organic beings, and had even obtained faint glimpses of the transformation of specific forms. But it was left to you to show, almost to demonstration, that the variations which species of plants and animals exhibit, and in natural selection through the struggle for existence, we have causes at once natural, universal, and effective which of themselves are competent not only to explain the existence of the present races of living beings, but also to connect with them, and with one another, the long array of extinct forms with which the palæontologist has made us familiar.

Farther, the Yorkshire Naturalists are anxious to place on record their firm conviction that in the care, the patience, and the scrupulous conscientiousness with which all your researches have been conducted; in the ingenuity of the experiments you have devised; and in the repeated verifications to which your results have been submitted by your own hands, you have furnished an example of the true method of biological inquiry that succeeding generations will deem it an honour to follow, and that cannot but lead to still further conquests in the domain of organic nature.

In presenting this small tribute of their high regard and esteem, the members of the Yorkshire Naturalists' Union cannot but hope and pray that many years of happiness and usefulness may yet remain to you, and that our Science and Literature may be still further enriched with the results of your researches.

(Signed) WILLIAM C. WILLIAMSON, F.R.S., President,
H. C. SORBY, LL.D., F.R.S., Vice-President,
GEORGE BROOK, ter. F.L.S., Secretary,
WM. DENISON ROEBUCK, Secretary,
and Eleven other representative Officials.

THE ATOMIC WEIGHT OF BERYLLIUM

FOR some time chemists have been doubtful what value to assign to the atomic weight of beryllium. Some years ago Prof. Emmerson Reynolds determined the specific heat of this metal to be 0·642; this number multiplied into 9·1 gave 5·8 as the atomic heat of beryllium; in other words it confirmed the generally accepted atomic weight. In 1878 Nilson and Pettersson re-determined the specific heat of beryllium, and found the number 0·408 for the temperature interval 0°–100°; hence these chemists concluded that the atomic weight of the metal must be increased by one-half ($13·6 \times 0·408 = 5·6$). If $Be = 9·1$ the oxide of beryllium is BeO, and the metal is placed in the magnesium group; but if $Be = 13·6$ the oxide is Be_2O_3, and the metal is placed in the aluminium group. But there is no place in Mendelejeff's classification of the elements according to the magnitude of their atomic weights for a metal with the atomic weight 13·6, forming an oxide M_2O_3, and exhibiting the properties of beryllium. The value of Mendelejeff's classification is however so great that chemists were not inclined to alter the atomic weight of beryllium except upon most cogent evidence.

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